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Biogeosciences Discussions

Interactive comment on "Co-evolution of phytoplankton C:N:P stoichiometry and the deep ocean N:P ratio" *by* T. M. Lenton and C. A. Klausmeier

Anonymous Referee #1

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General comments:

In this paper, modified versions of two recent models for nutrient cycling in the ocean are used to assess the effects of non-Redfield stoichiometry of phytoplankton and limitation of N-fixers by other resources than P on deep ocean N:P. The authors conclude that:

(1) Deep ocean N:P is set by the N:P ratio that triggers N-fixation, which is determined by the N:P ratio of non-N-fixers.

(2) Over the past 1 Gyrs, decreases in phytoplankton N:P and C:P ratios may have

driven a decrease in deep ocean N:P via increasing PO4.

(3) Even when they are only present in restricted areas of the world ocean, N-fixers can still regulate deep ocean N:P.

(4) Phytoplankton C:N:P and deep ocean N:P may have co-evolved.

The topic of this paper is important: there is strong evidence that the average stoichiometry of phytoplankton may have varied over earth's history and this may have had important consequences for the cycles of nutrients and carbon in the ocean. Model studies are the only way to understand and quantify these consequences adequately. The approach taken is a logical one: by using two existing models, the authors can build on previous studies and limit the time spent on general details.

A major problem with this paper is that it is very hard to read. This particularly holds for the description of the LW model. Also, many parts of the results and discussion sections suffer from a lack of sufficient detail and explanation, making it hard to understand how the authors reached their conclusions and nearly impossible to fully evaluate their validity.

In particular, the major conclusion that phytoplankton C:N:P stoichiometry and the deep ocean N:P ratio co-evolved is seemingly in contradiction with the statement that increased weathering and less removal of P on Fe-oxides led to an increase of deep ocean PO4 and corresponding decline in deep N:P. So who is driving what? It seems to me that organisms are not doing it alone. Also, it is not made sufficiently clear how the N:P ratio of non-N-fixers determines N-fixation.

I recommend major revision.

Specific comments:

Model description: 1. p1027. line 11. What are the units of this new production?

2. p1027. line 13-p1028 line 3. This section where equation (2) is introduced is difficult

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to understand without the original paper. Some more details would be useful. For example, why is O2 and Co mentioned here before it is used in an equation?

3. p1028. line 4. Denitrification also depends on Co/C, not only on the anoxic fraction of the ocean. This should be described in the text.

4. p1028. line 10 +18. It would be useful if the authors explain all the terms of the equations (4) and (5) fully in the text.

5. p1027-1031. A table with a list of relevant parameters of the LW and TT models would be useful

Results and discussion.

6. p1032-1033. section 3.2. It would be useful if the authors explain in more detail how they derive when NO3 is limiting and when PO4 could be/is limiting.

7. p1033. lines 8-22. Could this be summarized in a table or graph?

8. p1034. lines 6-7. It is not directly clear why PO4=226/(rC:P+kp).

9. p1034. line 23. It is not clear why it follows from the above that the N:P ratio of the non-fixers sets the N:P ratio for N-fixation.

10. p1035. lines 3-5. Where can I see the results of the TT model that confirm this conclusion?

11. 1035. It would be useful if the equations used to calculate NO3 and PO4 concentrations in Table 2 were given.

12. p1036. line 8. What is the explanation for the higher N:P ratio with increased weathering?

13. p1038. lines 9-10. It is not clear what the justification is for the conclusion that the N:P uptake of other plankton determines the N:P threshold for N-fixation (see point 9).

14. p1039. lines 26-27 and p1041. If weathering drives the increase in PO4 concen-

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trations, how can organisms then be responsible? This conclusion does not seem to be based on the model results.

15. p1040. lines 18-21. Where can I see these results showing that new production will be fairly constant? Over what period was this the case?

Interactive comment on Biogeosciences Discuss., 3, 1023, 2006.